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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Characteristics of sub-10 nm particle emissions from in-use commercial aircraft observed at Narita International Airport" *by* Nobuyuki Takegawa et al.

Anonymous Referee #1

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Review of "Characteristics of sub-10 nm particle emissions from in-use commercial aircraft observed at Narita International Airport" by Takegawa et al.

This paper describes measurements of aircraft engine particle emissions during takeoff operations at Narita International Airport. Concentration measurements are made with two TSI condensation particle counters (CPC) with differing lower detection size limits (3 nm for the Model 3776 CPC and 7-10 nm for the Model 3771 CPC), and the difference between the particle concentrations measured by these counters is intepreted as the number concentration of sub-10-nm particles. In addition, a TSI Scanning Mobility Paricle Sizer (SMPS) and Engine Exhaust Particle Sizer (EEPS) are used to measure

the size distribution of particles. A heated tube at 350 degrees Celsius is used to remove volatile particles so that the counters and SMPS can switch between measuring all particles or only the non-volatile particle fraction. The main finding of the paper is that there are significant differences between the particle concentrations measured by the 3776 counter versus the 3771 counter. Size distribution measurements for particle sizes greater than 10 nm are also presented to support the hypothesis that a significant fraction of the total and non-volatile particle number concentrations are below 10 nm; however, as the authors note, there are substantial particle diffusional losses at these sizes and the uncertainties and data corrections are significant!

Overall, the manuscript is well written and enjoyable to read. The underlying data are available in the supplementary information, which is excellent. The paper does a great job of characterizing the detection and penetration efficiences of the particle counters (although, I have a significant quibble with the use of the 3772 CPC to characterize the latter as discussed below). I previously reviewed a prior version of this manuscript for another journal, and I'm delighted to see that the authors have incorporated many of my comments/suggestions from that review into the present manuscript.

Observational reports of aircraft engine particle emissions in the literature are fairly limited given the large diversity in aircraft/engine types and airport conditions, and thus, this study is valuable in helping to overcome the current paucity of data. The content is appropriate for Atmospheric Chemistry and Physics. The paper may be publishable, but only if the following major comments are satisfactorily addressed:

1) On Lines 21-24, Lines 314-318, and elsewhere, the manuscript implies that it is somehow significant that the total particle number exceeds the number of non-volatile particles and that the regulatory emissions are somehow not accounting for these particles. This mischaracterizes the rationale behind the engine certification testing, which is designed to evaluate the emissions contributions from different engine types under relatively controlled conditions. It is well known that the volatile particle fraction is highly variable and depends on numerous variables including the fuel sulfur content and the

environmental temperature. The regulatory focus on non-volatile particles attempts to remove some of this variability; although, there are still fuel composition impacts on soot formation that need to be accounted for. In sum, the comparison that the authors are making here is not an apples-to-apples comparison and is misleading. These sentences should be removed.

2) On Lines 25-26 it is stated that the mode diameters of the size distributions were found to be smaller than 10 nm in most cases, but this does not seem to be well established from Figure 9 (there are multiple curves where there is a discernable mode around 20 nm).

3) On Lines 27-29, it is suggested that the present paper "provides new insights into the significance of sub-10 nm particles..." that are important for human health and aviation emissions inventories. I'm not sure what these puported new insights are. The present study seems to be confirming extensive past literature that has found large emissions of volatile particles (thought to be organics and sulfuric acid), but these particles may or may not have a significant impact on health. This impact would depend on their solubility – if they are soluble, then the health impact would follow dose toxicity (which would be pretty insignificant). If they are insoluble, then they could penetrate the lungs and be important. Not all ultrafine particles are created equal here. Regarding the second point about emissions inventories – how important are these particles? They are likely to be rapidly depleted via coagulation processes, and so the number-based emissions of these sub-10 nm particles are likely to be very different even at the end of the runway as compared to the surrounding area. The strength of this statement regarding the impact of the present study needs to be toned down considerably.

4) I don't think there is support for the statement made on Line 34 that aircraft emissions somehow don't participate in any wet removal processes.

5) I agree with the authors' statement on Line 70 that the technical issues associated with particle transport and losses of sub-10 nm particles need to be properly con-

sidered. How are these technical issues addressed in the present study? On Line 128-129, it is mentioned that the diffusional loss corrections within AIM are used, but these only apply to the SMPS system itself (not the 3m sampling lines or other flow splits and particle treatments). What corrections were applied to the concentration and size distribution data? Do these corrections drive the conclusions of the present paper, or are the findings the same even if the corrections are neglected?

6) Is it reasonable to assume that the particle residuals leaving the thermaldenuder are 1 nm? What about if they were 3 nm? or 5 nm? How robust are the paper's findings to this major assumption?

7) I think it's great that the detailed removal efficiency tests described on Lines 221-222 were completed, but I question the use of the 3772 CPC as the detector since it doesn't rule out the possiblity that the particles didn't completely evaporate and would still be detectable by the 3776 CPC. If possible, it would be important to redo these experiments with the 3776 since the difference between the two CPCs is being used to infer the presence of sub-10-nm non-volatile particles.

8) I don't understand what is being referred to by the statement on Line 293 about the abscence of an "artificial nucleation mode". Please clarify.

9) On Line 381 replace "soot" with "non-volatile"

10) On Line 386, strike the "s" from the word "evidences"

11) The sentence on Lines 391-393 speculating about rapid dilution prempting the growth of soot particles is unfounded and should be removed.

12) The discussion on take-off particle number concentration impacts on aircraft cruising altitudes on Line 423 does not seem relevant to the present paper.

13) Please change the y-axis scaling for the lower-left panels of Figures 7-8 to a linear scale to clearly show the agreement between the measurements of the smaller number mode. The contribution of the larger modes are already well captured by the dV/dlogDp

plot.

14) From the inset of Figure 9a it looks like there's the beginning of a hump in the gray curves that is reflected by the black line, but in Figure 10c this doesn't seem to be the case. What is the arbitrary units scale in Figure 10c and how were these different quantitative data scaled together?

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